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Action research is often criticized for not being properly based in objective facts or for not formulating testable theories, in short, for not being properly scientific. But with what kind of science should it be contrasted? Hanson (1958) distinguishes between finished, (classical) sciences and research sciences. Unlike a finished science that can be conducted by us as individuals within an already well formulated disciplinary discourse, a research science cannot. If it is to inquire into *possibilities not yet actualized*, it must be conducted in a much more situated, conversational manner. Thus as researchers, instead of functioning as detached observers, seeking to discover the *invisible* or 'hidden' causes of an observed event, we must operate in an ongoing real-time situation in a much more *dialogical* manner. For such dialogically-structured activity can, within the dynamics of its unfolding, give rise to *transitory understanding*s and *action guiding anticipations* of a 'situated' kind, thus enabling all those involved in such activity to 'go on' with each other in unconfused ways. It is this participation in a shared *grammar* of felt, moment by moment changing expectations that are – in the interests of a decontextualized objectivity – precluded (or 'lost') within the disciplinary discourses of a finished science. Thus, guided by Wittgenstein's (1953) writings in his later philosophy, I want to show in this article that, not only is it more accurate to compare action research with research sciences than with classical sciences, but that action research can find its intellectual legitimacy in the same sphere of human conduct as all of our sciences – in people being responsibly accountable for their own actions to the others around them in terms of their immediate relations to their shared surroundings.

Key words: Wittgenstein, the background, *ways* of seeing, action guiding anticipations

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This special issue of the IJAR is aimed at improving the understanding of action research (AR) within the academic social science community. Again and again we find that mainstream social scientists have little knowledge of, and almost no experience with, action research. However, in discussions on AR, they still feel free to make judgements as to its inadequacies and to make authoritative pronouncements on what they suppose the 'proper' nature of research should be. This results not only in many misunderstandings and/or silences between mainstream social scientists and action researchers, but (often) leaves action researchers also feeling treated as intellectually inferior to those engaged in 'proper' research, and uneasy in being observed by them. Indeed, it is easy for practitioners to feel that, although there is a crying need for their skills, their more 'pure' colleagues on the side-lines will surely, with their 20/20 hindsight in their 'after-the-fact' analyses, always find fault in what they 'did for the best' in the circumstances in which they worked. This unhappy state of affairs is has gone on for far too long.

But more than the political tensions and uncomfortable emotions involved, there are two other consequences of even greater importance: (1) One is that the classical paradigm of scientific research, based in a Cartesian-Newtonian vision of the world in which we live, is still prevalent in much research in the social and behavioural sciences. And this has, due to its 'theory-driven' nature, diverted our attention away from the nature of our *practices*. Thus the whole realm of practice has remained relatively unexamined and thus intellectually impoverished – in treating practice as merely the realm in which theories are 'applied', the realm of practice has been left to 'take care of itself', so to speak. Only recently have efforts been made in the academy to remedy this fact (e.g., Schatzki 2002; Schatzki *et al.* 2001). Indeed, most crucially, we are now beginning to see that our practices are a part of 'the background' (see the next section below) that makes our theoretical, representational forms of talk possible. (2) There is also a second consequence: While the undoubted successes of 'scientifically' conducted research can be cited in academic arguments in support of 'classical' research methods, it is difficult to assess the *disadvantages* of such 'scientifically' conducted research without the existence of a visibly-rational account of a competing mode of disciplined inquiry in terms of which to construct reasoned contrasts.

My aim, then, in this article, is not to set out a comprehensive intellectual justification for action research, but to show: (1) following Hanson (1958), that unlike classical, 'finished' sciences rooted in and modelled on "past scientific achievements" (Kuhn 1962, 1970: 10), a *research science* must be rooted in the same forms of accountable human communication that ground all our practical dealings with each other in our daily lives, and (2) that research conducted in accord with classical modes of 'scientific' analysis results in 'losing the phenomena', i.e., the modes of informal conversational communication that are crucial to the conduct of our everyday human affairs.

Foregrounding our 'background' practices

Although we are always immersed in them, like the proverbial fish being the last to discover water, it is only recently, with the work of such thinkers as Wittgenstein (1953), Merleau-Ponty (1962), Garfinkel (1967), Bakhtin (1981, 1986), and Voloshinov, (1986), amongst others, that the character of the spontaneously occurring forms of talk and action, of speaking and listening, of practical understanding, etc., routinely at work in our everyday, conversationally-structured activities, has come to our intellectual attention. In all our classical accounts of scientific method, our everyday life practices have remained unnoticed, in the background. The hope has been that, one day, our understanding of human phenomena will have advanced sufficiently to 'explain' them (in terms of a representational theory). Gradually, however, with the work of Searle (1981), Taylor (1993), and Dreyfus (1991), 'the background' is beginning to be foregrounded in our intellectual inquiries.

Searle (1981), for instance, describes its nature thus: "The Background is a set of nonrepresentational mental capacities that enable all representing to take place. Intentional states only have the conditions of satisfaction that they do, and thus only are the states that they are, against a Background of abilities that are not themselves Intentional states. In order that I can now have the Intentional states that I do I must have certain kinds of know-how: I must know how things are and I must know how to do things, but the kinds of "know-how" in question are not, in these cases, forms of 'knowing that'" (Searle (1981: 142). While Wittgenstein (1980: 16) remarks: "Perhaps what is inex-

pressible (what I find mysterious and am not able to express) is the background against which whatever I could express has its meaning."

In my estimation, it is the great power of Wittgenstein's 'philosophy' that he has outlined a set of methods that enable us to come to an understanding of a least some aspects of our activities in this sphere, to grasp the nature of some of our own human 'doings' from within the middle of our doing of them. His philosophy, then, is of a practical-descriptive kind: that is, rather than being aimed inwards towards thinking, *prior to any action*, about which features in a particular subject-matter we should approach or address in our inquiries, it is aimed outwards toward helping us become more actively attentive towards previously unnoticed aspects of our surroundings of possible relevance in the shaping of our actions.

This, clearly, is a very different kind of goal from the theoretical goals pursued in the classical, metaphysical philosophies of the past. Indeed, he wants in his investigations "to replace wild conjectures and explanations by the quiet weighing of linguistic facts" (1981, no. 447)[1] thus to produce merely a *description* of the facts that matter in the issue concerned – a description which, if one was initially intellectually disoriented, if one did not know what was possible as a next practical step, would *justify* saying to those around one (at least for the immediate practical purposes in hand): "Now I know how to go on" (1953, no.154). His investigations are thus, not at all aimed at developing explanatory theories to the nature of the world around us and of our possible knowledge of it, but at alerting us to what in fact is occurring in our involvements with each other, and with our surroundings (which makes such theorizing *possible*) – a perceptual rather than a cognitive aim.

In other words, he works – like an action researcher – *from within* our already existing practices with the aim of seeking previously unnoticed openings for their further refinement, elaboration, and correction.

In an earlier very short article (Shotter 2004), aimed at outlining Wittgenstein's philosophical practice and its relation to action research, I noted Greenwood's (2002: 117) account of academic social sciences as "primarily internally regulated, university based, professional activities" that "privilege

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[1] From now on, all date only citations are from Wittgenstein's works.

'theory' and 'method' over all else... [in ways that were] quite out of step with the meanings of these terms in the physical and natural sciences" (ibid. 119). And I noted that he went on to conclude: "From what I have written, it seems that action research should dominate the social sciences. It has methods that are far more 'scientific' in the sense of knowledge tested and refined in action. It mobilizes relevant knowledge from people in a position to know their condition far better than conventional research can with its extractive approach... And it is driven by strongly-held democratic values" (ibid. 128 f.). And I agreed with him.

Why doesn't it then in fact occupy a dominant position in the social sciences? Greenwood suggests two reasons: "suppression by the social sciences and political elites and the sloppiness and negligence of action researchers themselves" (ibid. 129). However, in my earlier article I suggested, and want to suggest even more forcibly here, that while what Greenwood suggests may be to an extent true, there is, I think, a third still more central reason, a reason that motivates both the suppressive tendencies he noted, as well as the lack of intellectual rigour of action researchers themselves: it is, as Wittgenstein (1953) suggests, that we are victims of the "bewitchment" (no. 109) that our use of words can work on us. In other words, unnoticed by us, but present in our 'official', everyday, taken-for-granted ways of spontaneously communicating with each other, are *ways* of 'looking at' (perceiving), thinking about, acting toward, talking of, and valuing what we take the 'things' around us to be which could quite easily be otherwise than we currently take them to be.

Hanson's Wittgensteinian philosophy of *research* sciences

In a moment, I will turn to Wittgenstein's more general investigations into the unnoticed, background influence of the words we use in shaping our thought and talk in our more everyday affairs, our spontaneous ways of 'making sense of' events in our surroundings, but here let me focus on Hanson's (1958) account (as a student of Wittgenstein's) on the background *ways* of looking, thinking, and speaking into which we are trained as scientists. For he wants to argue that *that* kind of training might not have been as useful to us as is often claimed.

Hanson's focus on our ways of seeing

Hanson (1958: 1) begins his inquiries by noting that philosophers "have regarded as paradigms of physical inquiry not unsettled, dynamic, research sciences like microphysics, but finished systems, planetary mechanics, optics, electromagnetism and classical thermodynamics." And it is the methods of such finished or 'classical' sciences as these that have been taken as paradigmatic of how a 'proper' science – a science that all will agree qualifies for that title – should be conducted. Indeed, as he goes on to note, past philosophers of science have often suggested that "'when micro physics settles down it will be like these polished systems'"(ibid. 1), thus nothing is lost, it seems, in taking them as our guides in inquiring into how research should in fact be conducted. But this is a mistake, he says, and if this attitude is accepted, the more disorderly, the more unruly, conversational aspects of microphysics *as a research science* will be lost. Indeed, as Hanson (1958) notes, although 'classical' sciences "are not research sciences any longer, ... they were at one time." Thus: "Distinctions which at present apply to them ought to be suspect when transferred to research disciplines: indeed, these distinctions afford an artificial account of the kinds of activities in which Kepler, Galileo and Newton themselves were actually engaged" (ibid. 1). In other words, to look at the *history* of the now 'classical' sciences through the lens of their 'finished' nature might be to distort, or to lose, the very phenomena responsible for the processes leading to their successful development.[2]

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[2] Interestingly, Kuhn (1962, 1970) later took this very approach to The Structure of Scientific Revolutions that Hanson had suggested was mistaken: he took already established sciences as being of central importance. As he saw it, the function of such paradigms was in the conduct of what he called normal science – where research is "firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice" (ibid. 10). And it is the study of these paradigms (of finished sciences – Hanson) that, he said, "mainly prepares the student for membership in the particular scientific community with which he will later practice" (ibid. 11). Thus in Kuhn's (1970: 52 f.) view – with his interest in the history (but not the philosophy) of scientific revolutions – training scientists in normal or finished sciences is central, because such a training can give rise to "paradigm induced expectations"; and: "Discovery commences with the awareness of anomaly, i.e., with the recognition that nature has somehow violated the paradigm-induced expectations that govern normal science. It then continues with a more or less extended exploration of the area of anomaly" (ibid.

In short, all the human phenomena involved in, and surrounding, our inquiries into *possibilities* (many of which are, ultimately, not realized) are lost. In the interests of achieving an objective, decontextualized, generalized account, independent of any unique events or unique individuals, all the fleeting, only "once occurrent events" (Bakhtin 1993: 1), in which we conduct *exploratory orientations* toward the yet-to-be- determined phenomena confronting us, are eradicated. Such 'relational experiments' – for they are to do with the tentative adoption of different *ways* of relating ourselves to the others and othernesses[3] around us – are, as Hanson (1958) shows, a crucial part of a research science. But the need for, and role of, such orientational explorations can play no part in such polished and finalized accounts of what a 'proper' science *is*.

However, as Hanson (1958) cautions, in outlining what is involved in studying dynamic, research sciences, "the issue is not theory-using, but theory-finding." Thus, he says: "let us examine not how observation, facts and data are built up into general systems of physical explanation, but how these system are built into our observations, and our appreciation of facts and data" (ibid. 3).

What Hanson brings into the foreground of our consideration, then, is not only how all the attitudes, compulsions, urges, inclinations, etc., that usually lie in the background of our research activities which can – without our realizing it – influence *what* we see, *and* our ways of talking and acting in relation to what we see. But even more importantly, how, in the moment of our looking over what is before us, someone's talking (and their other activities, such as pointing and gesturing) can be intertwined into our looking to help us organize what we see into a unity. For, as we realize, we do not 'take' pictures with our eyes in an instant as a camera takes pictures; our eyes, in their saccades and fixations, dart about within a visual field as we 'look over' it, and thus we gather only fragments of data here and there. To integrate or to organize them into a visual unity or whole – the seeing a 'something' within

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52 f.). Kuhn's account of the workings of a research science as thus very different from Hanson's. In Kuhn's terms, Hanson's scientists are inevitably functioning in a pre-paradigmatic realm of inquiry.

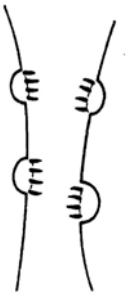
[3] That is, the 'still undetermined somethings' awaiting a linguistic designation.

a 'situation' – we require a *way* of looking, a way of sequencing our looks such that the seeing of one aspect of a whole arouses in us an expectation that, were we to look for another aspect of it, there is a chance of finding it present also.

In other words, there is an extra-sensory element in vision, an organized *way* of looking that leads us, as we 'look over' a visual scene, to 'see that' it is an X rather than a Y that is before us. It is his concept of '*seeing that*', and his emphasis on the intertwining of our talk into our seeing at the moment of our looking at what is to become 'a something' for us, that is crucial.

He illustrates the working of this influence, and its results, by use of the following figure 1:

Figure 1:



About figure 1, he remarks: "Your retinas and visual cortices are affected as much as mine are; our sense-datum pictures would not differ. Surely we could all produce an accurate sketch of figure 1. Do we see the same thing? I see a bear climbing the other side of a tree" (pp. 12-13). For those who have never seen this drawing before, his talk of 'a bear' can produce an 'oh yes, now I get it' reaction.

The point is, not so much that the word 'bear' gives us a particular picture (a mental representation) of an entity, but that the word 'bear' arouses in us all sorts of transitory understandings and action guiding anticipations (Bakhtin 1986, Shotter 2005), so that we are able, in practice, not only to know 'where' we *are*, so to speak, but also how to 'go on'[4] to entertain a whole range of further *specific* possibilities (but not just any). Thus, as Hanson re-

[4] As we shall see, Wittgenstein (1953) talks of understanding as being, not something that happens mysteriously inside someone's head, but as something that a person becomes able, in practice, to do: "Try not to think of understanding as a 'mental process' at all. – For that is the expression which confuses you. But ask yourself: in what sort of case, in what kind of circumstances, do we say, 'Now I know how to go on', when, that is, the formula has occurred to me?" (no. 154).

marks, "seeing a bear in figure 1 [is] to *see that* were the 'tree' circled we should come up behind the beast" (ibid. 20), and (most likely) see the bear's head towards the top of the tree and its tail towards the bottom of the tree (for bears do not usually climb up or down trees with their bodies inverted), and so on. In other words, by our use of certain words at an appropriate moment, we can not only draw people's attention to a particular feature of their surrounding circumstances, but arouse in them a range of action guiding anticipations as to where they might look next for the next *possible* events occurring in the situation depicted. He further points out how our talk intertwined gesturing (in words or in other of our actions) towards aspects of a shared (visual or other) circumstance can also influence our *ways of looking*, thus to see a meaning in an aspect of our circumstances.

Ochs et al. (1996) describe research situations in the physics in which just such activities occur. In a discussion amongst a set of solid state physicists researching into phase transitions, a researcher moves towards a diagram on the blackboard depicting phase change transitions and, while pointing first to the right of the diagram and then to the left, says: "When I come down I'm in the domain state" (Ochs et al. 1996: 331). The others in the room 'get it' and begin to raise further questions about phase transitions: "Overwhelmingly, interlocutors are looking at, gesturing towards, and/or touching locations within these graphic spaces as they say utterances such as 'And now I continue down in temperature', 'If I come in this way (0.5 sec pause) and go he:re there's no decay', or 'Why don't I go: to the long range ordered phase in the Kleeman experiment'" (ibid. 357).[5] Indeed, the physicists seem quite incapable of communicating *without* jumping up to interweave what they have to say with pointings at and/or sketchings on the blackboard.

As Ochs et al. (1996) point out, the interlocutors are living in an unsettled, multi-dimensional, *liminal* context, i.e., a context which is still only on the threshold of being determined. Thus, in the unfolding movement of the laboratory discussion, it is still possible to direct each other's attention to many different possible aspects of the relevant phenomena, all currently co-present, any one of which *might* at some point be of importance in determining the fi-

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[5] : indicates the stretching of a vowel sound, while ___ indicates emphasis.

nal outcome of their research. And it is only in the little dramas that they stage, that they can create between them *at each moment in the context of their discussions* a sufficiently determinate sense of 'where' they are and what their 'next step' might be, that they can all as research physicists agree, as to what their current laboratory results show with respect to what their next experiments should be.

In other words, in 'seeing that' the fragments (parts) presented in the illustrations *point toward* a certain whole – in the context of which they have their *specific* meaning – we are able in a kind of hermeneutical, part-whole process, to anticipate what we might see in our further visual involvements with the entity in question. For instance, Hanson (1958) notes: "Seeing a bird in the sky involves *seeing that* it will not suddenly do vertical snap rolls [i.e., a stall leading into inverted flight],[6] and this is more than marks on the retina. We could be wrong. But to see a bird, even momentarily, is to see it in all these connections... every perception involves an aetiology and a prognosis" (ibid. 21, my emphasis). It is both this placing of an event into a temporal, before and after context, *and* being able to responsibly account to others for that placing – as Ochs et al.'s (1996) physicists do – that is crucial to the creating of a new *way* of seeing among a group of co-participants and co-researchers.

Frontier thinking

So: Although a number of people may confront a certain event, would they all see 'the same thing'? No, we must answer, for "the *ways* in which they are visually aware are profoundly different. Seeing is not only the having of visual experience; it is also the way in which the visual experience is had" (Hanson 1958). In other words, what is often at issue between scientists in a research science, is not so much how to describe (represent) its current subject matter accurately; it is a matter also of saying also *what* next it *might be* – a difference between not what the facts currently *are*, but a matter of the different ways in which they might all 'hang together' in constituting a larger whole, thus to give each its *meaning* in relation to that whole.

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[6] Hanson was a qualified aeroplane pilot and knew about such things as 'snap rolls'!

To bring out what he means here, Hanson compares Mach's use of a formulaic proposition in carrying out a calculation with Hertz's use of it. Both would, he shows, get exactly the same answer. But while Mach "construed dynamical laws as summary descriptions of sense observations," Hertz treated them as "highly abstract and conventional axioms whose role was not to describe the subject-matter but to determine it" (ibid. 118) – the difference between an 'after-the-fact' (Mach) and a 'before-the-fact' (Hertz) *use* of the formula. This would mean that, "though they get the same answer to the problem, the difference in their conceptual organization guarantees that *in their future research* they will not continue to have the same problems" (ibid. 118, my emphasis). The difference between them – to do with the connections and relations they sense as existing within the phenomena of their inquiries – would show up "only in 'frontier' thinking – where the direction of new inquiry has regularly to be redetermined" (ibid. 118).

These differences might seem to be subtle differences mattering very little in practice. And indeed, as Hanson (1958) shows, in doing calculations, making predictions, and in providing explanations when working with scientific formulae, these two scientists might not differ at all. But, as Hanson (1958) makes clear, to try to force the thinking of workers in a research science into the mould of classical text-book sciences (in which creativity is supplanted by systematizing), when they are in fact *creating* new possible forms of research, is to mislead ourselves into thinking that new research is merely a matter of rearranging old facts into new formal patterns, into thinking that it is a task, essentially, of a quantitative kind. Whereas, the task of making a unique, once-only, first-time sense of a bewildering phenomenon, is a task of quite a different, qualitative kind – it is, as we shall see, not so much a difficulty of the intellect, i.e., a problem that can be *solved* by thinking, as a difficulty of the will, which needs *resolving* in a struggle of a more practical kind (see Wittgenstein 1980: 17; and the discussion below). For it is to do with changing our taken-for-granted, 'background' ways of making sense of the phenomena before us. But again, it is not enough for an individual to make such a change merely within themselves; they must both be able to instruct others in the new way of seeing, and to give such others good reasons, i.e., to responsibly account, for such a way of viewing the phenomena in question.

Wittgenstein's ways with words

Modern philosophy has always been in the thrall of science. It is only by following "the secure path of a science," said Kant in his *Critique of Pure Reason* in 1787, that philosophy can become more than "a merely random groping" (Kant 1970: 17). And even now, we are still under that spell, the belief that there is a form or pattern of reasoning, a methodology, that we must follow if we are to overcome the difficulties we face in our lives.

Difficulties of the intellect versus difficulties of the will

Wittgenstein's (1953) great achievement in his later philosophy is to have broken this spell, to have made it very clear to us that many of our difficulties are *not* of the form of *problems* that can, by the application of a science-like methodology, be solved by reasoning; nor are they are not "empirical problems" that can be solved by discovering something currently unknown to us. They are difficulties of a quite another kind: they are difficulties *of the will* rather than *of the intellect*; that is, they are orientational or relational difficulties, to do with the how we spontaneously respond to features in our surroundings with appropriate expectations and anticipations as to how next to 'go on' with our activities within them without (mis)leading ourselves into taking an inappropriate next step[7].

> "What makes a subject hard to understand – if it's something significant and important," he says (Wittgenstein 1980: 17), "is not that before you can understand it you need to be specially trained in abstruse matters, but the contrast between understanding the subject and what most people *want* to see. Because of this the very things which are most obvious may become the hardest of all to understand. What has to be overcome is a difficulty having to do with the will, rather than with the intellect."

Thus, if we are to overcome difficulties of this kind we must work on our ways of seeing things, on what we expect of them. For, as he makes clear to us, our ways of talking can exert powerful, directive effects on us, unconsciously, without our being easily able to deliberate on how we might, in the

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[7] "A philosophical problem has the form: "I don't know my way about" (1953, no. 123).

course of our thought and talk, take pause, and seek alternative paths to those seemingly indicated by our current ways of 'going on'.

Such difficulties, he suggests, can be overcome "by looking into the workings of our language... in such a way as to make us recognize those workings: in despite of an urge to misunderstand them. The problems are solved, not by giving new information, but by arranging what we have always known. Philosophy is a battle against the bewitchment of our intelligence by means of language" (1953, no. 109).

Wittgenstein's positive' grammatical investigations

Our ways of thinking, talking, and acting are too familiar to us. We are not very aware of the part our words can play in giving shape to, or in organizing, our thinking, talking, and acting. Although we have talked in the past of thought as calculation, or as information processing, if Wittgenstein (1953) is correct[8], then we must think of our words, or better, our speaking – the inner mental movements we make in the uttering of our words – as being a very important medium of thought also.[9]

In his 'grammatical' investigations, he has focussed our attention on at least two aspects of their role in our thinking: 1) Negatively, so to speak, on the *bewitchments* they can mislead us into in our unreflective uses of our words; but also (2) positively, on how we can come to a clear understanding of an actual occasion of word-use by means of an appropriate *description* of

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[8] Along with many others, of course, notable among them being Vygotsky (1978, 1987), Bakhtin (1981, 1984, 1986), and Merleau-Ponty (1962, 1968).

[9] Elsewhere (Shotter 2005), I have emphasized the importance of Bakhtin's (1981, 1986) relationally-responsive account of our understanding of people's words *as they utter them* (in contrast to the usual representational-referential account of their meaning once uttered). For, among the many other features of such responsive talk, is its orientation toward the future: "The word in living conversation is directly, blatantly, oriented toward a future answer-word," he says (Bakhtin 1981: 280, my emphasis), "it provokes an answer, anticipates it and structures itself in the answer's direction. Forming itself in an atmosphere of the already spoken, the word is at the same time determined by *that which has not yet been said but which is needed and in fact anticipated by the answering word*. Such is the situation of any living dialogue." Wittgenstein (1953) notes this also: "It is in language that an expectation and its fulfilment make contact" (no. 445).

it: "Philosophy may in no way interfere with the actual use of language; it can in the end only describe it" (no. 124). Here, I will focus almost wholly on his 'positive' investigations.

In Shotter (2005), I noted Bakhtin's (1981) remark that: "Forming itself in an atmosphere of the already spoken, the word is at the same time determined by that which has not yet been said but which is needed and in fact anticipated by the answering word" (Shotter (2005: 280). In other words, among the many other features of our spontaneously responsive, everyday talk, is its function in orienting us toward the future, thus to provide us with *ways* of organizing, or 'orchestrating', fragments of thought and talk, action and perception, etc., etc., into intelligible wholes. What Wittgenstein (1953) adds to Bakhtin's general point here – in what he calls his grammatical investigations – is that if we attend to our *actual use of words*, we find that we use them in countless different ways, and our uses are both complex and uniquely intertwined in subtle detail into the deeds and actions of our lives.

When considered in the abstract, as we do when theorizing or doing philosophy, this complexity can be overwhelming. But what Wittgenstein shows us is that, if we pay close attention to particular details noticeable in particular concrete circumstances – to tones of voice, bodily movements and gestures, facial expressions, eye direction, and so on – and thus place our everyday utterances back into the circumstances of our everyday lives then, rather than bewildering us, the detailed relations between particularities within the complexity can arouse quite specific responses in us. He describes our changed way of attending to relevant phenomena thus: "we are tempted to say 'only this can be really seen' when we stare at unchanging surroundings, whereas we may not be at all tempted to say this when we look about us while walking" (1965: 66). He wants to consider our use of words "from within" the *movements* involved in our saying of them, rather than from the outside when we look only at the static forms of the words, i.e, what we finally *said* in our utterances. When we can survey the actual step-by-step movements made by a person (their 'expressions'), in relation to each aspect of the 'terrain' to which these movements were related, then it becomes obvious to us why the person as he or she did.

For instance, in Katz and Shotter (1996), we were concerned to study what could be 'heard' by doctors in a patient's voicing of her/his replies to the doctor's questions. While Katz (as a co-practitioner-researcher) was oriented toward noticing tones of 'personal concern' in a patient's voice, the doctor wasn't; the doctor's concern was with the *medical information* provided in what the patient said; the doctor was thus oriented toward the patient simply as a source of such information. This, however, occasioned a cool and tense exchange, with expressions of anxiety by the patient. Katz's suggestion to the doctor – that she attend to and respond to such expressions, and explore in some detail their meaning for the patient – worked to help the doctor create a very different kind of relationship, in which the medical examination proceeded in a much more warm, open, and worthwhile manner.

We can see what Wittgenstein (1953) is doing in his grammatical investigations in the same light: he is showing us how to display to ourselves, in the face of all the myriad forces that could be at work influencing us in what we do, how we might in fact achieve a *resolution* of them.

Thus, in short, in bringing "words back from their metaphysical to their everyday use" (1953, no. 116), he changes our point of view from that of a contemplative observer to that of an agent in motion; and by spelling out the small details of our movements, enable us to appreciate our movements as if in 'slow motion'. In so doing, we can begin to see how, in practice, we do in fact orient or relate ourselves to our surroundings, and it is this that dispels the need to hypothesize about special "mental processes" hidden inside our heads in attempts to account for our actual achievements. So, for instance, in discussing, say, feelings of *confidence*, instead of asking questions about the distinct brain states that might be involved, he attends to the actions and expressions out in the world: "Is our confidence justified? – What people accept as a justification – is shown by how they think and live. We expect this, and are surprised at that. But the chain of reasons has an end" (nos. 325, 326). Something that usually, as he says, "goes by so quick," we would like to see it instead "as it were laid open to view" (1953, no. 435), and this can be done by setting an action within the details leading up to it, and those following from it.

As I mentioned above, besides these more 'positive' grammatical investigations, besides these back and forth investigations into the diachronic links between words and their surrounding circumstances, Wittgenstein also investigates synchronic ones, the sideways linking of words simply with other words. Indeed, it is this particular aspect of language use, when people's talk is *not* uniquely intertwined into the deeds and actions of their lives (rife among the talk of academics and intellectuals), that is the main target if his investigations. For, as he sees it, "philosophical problems arise when language *goes on holiday*" (no. 38).

This divorcing of words from the practical contexts within which they can have a specific use, occurs when sensible forms of talk are uttered in situations – usually by people seated in classrooms, seminar rooms, or conference halls – begin to cudgel their brains with such questions as: "What actually *is* 'the scientific method'?" "What *is* practice?" "How do sentences manage to represent *this* rather than *that* state of affairs?," and so on. While we might be able to depict (picture, represent) a set of supposed entities that the words 'method', 'practice', and 'represent', and argue for the correctness of certain depictions over all others, no depiction as such would provide us with the *way* of seeing needed to anticipate seeing these activities in all their appropriate relations with their surroundings. As Hanson (1958: 21) notes, to repeat, "every perception involves an aetiology and a prognosis."

Wittgenstein (1953) seeks, then, in his grammatical investigations to provide us with methods for describing the precise sequential intertwining of our speech with other movements, as our actions unfold in time, that enables us to sort out the *actual* meaning of our words from their *possible*, i.e., their meanings when unrelated to any actual practical circumstances. He brings out what is at issue here in a discussion of the circumstances surrounding an act of pointing: While there might be what can be called "characteristic experiences" which seem to accompany acts of pointing, no one characteristic process occurs in all cases. "Besides," he adds, "even if something of the sort did recur in all cases, it would still depend on the circumstances – that is, on what happened before and after the pointing – whether we should say 'He pointed to the shape and not to the colour'" (1953, no. 35). In other words, as in the situation of the research physicists depicted above, the nature of events on

their own is indeterminate; only when they are considered in relation to their possible places within larger schemes of events, can their nature be determined – but that 'placement' is still not a simple activity, much reflection, exploration, and discussion with others of other possibilities is required before a group can be satisfied that it has a *way of seeing* the phenomena that is now to be the topic of their research.

'Losing the phenomena' and regaining them

Above, then, without attempting to set out a comprehensive intellectual justification for action research, I have attempted to show that research modelled on the methods of finished, classical sciences, fails to capture (and must always fail to capture) those unique moments of human communication creative of *new ways* of seeing (and hearing, etc.) among a group of researchers. Presenting the arrived at results of one's research in terms of a generalized theory representing a (possible) repetitive pattern in human events, inevitably occludes, erases, or 'forgets' the unique sequence of unique activities involved creating a new way of looking (or listening, etc.) from one fragmentary event to another (within a larger background situation) in such a way as to see (or hear) them all as constituting a nameable event.

What is special about such moments is, not only that they are unique and fleeting, and occur always for "another first time" (as Garfinkel 1967: 9, puts it), but that they are intertwined in a uniquely sequenced way into a speaker's embodied expression of certain words (and gestures, and other actions), *and* also into the occurrence of certain other events shared by both speaker and listeners alike. And it is especially the *specific* (as opposed to the general) *transitional understandings* and *action guiding anticipations* (the concrete 'positionings' and 'pointings') aroused by the precise use of certain words at certain moments in time that gives them their practical meaning – their capacity to (re)orient us toward new ways of seeing (and hearing). In other words, in speaking (and perhaps writing) out *from within* the ongoing activities within which we are currently engaged as researchers, we do not just 'point out' relevant phenomena to others in our group, but in our talk and gesturing we also 'point forward', prospectively, to the possible next events

that might occur – that is, as researchers in a research science we must provide not just accurate knowledge but also *actionable knowledge*.

Sciences rooted in and modelled on "past scientific achievements" (Kuhn, 1962, 1970: 10), and interested inachieving objective, decontextualized, generalized representations of states of affairs, independent of any unique events or unique individuals must of necessity '*lose the phenomena*'[10] relevant to creating such new *ways* of looking. They fail to arouse in the finalized theories they provide, the 'anticipations' that guide us in seeking an event's connections with its surroundings that motivated us in its origination as a science. Indeed, they do more, for in their production of finalized accounts of a state of affairs, they close off the road back to an understanding of these origins.

This point is not new. Perhaps one of the earliest writers to point it out was Ludwik Fleck (1935/1979). As he shows in his historical account of the development of the Wasserman reaction (for the detection of syphilis), once a practice is established and working well, we tend to project back into it, falsely, both a *clear* origin and an *orderly* course of development, neither of which in fact it had. As Fleck (1979: 86 f.) puts it: "If after years we were to look back upon a field we have worked in, we could no longer see or understand the difficulties present in that creative work. The actual course of development becomes rationalized and schematized. We project the results into our intentions; but could it be any different? We can no longer express the previously incomplete thoughts with these now finished concepts. Cognition modifies the knower so as to adapt him harmoniously to his acquired knowledge. The situation ensures harmony within the dominant view about the origin of knowledge. Whence arises the 'I came, I saw, I conquered' epistemology, possibly supplemented by a mystical epistemology of intuition."

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[10] I take the term 'losing the phenomena' from Garfinkel (2002), although Vygotsky (1987) also describes what is at issue very nicely: It can occur, he says, when we begin with "the decomposition of [a] complex mental whole into its elements...[where] its products are of a different nature than the whole from which they are derived... [This] results in products that have *lost the characteristics of the whole*..." (ibid. 45, my emphasis).

This is not, of course, an important issue in the medicine or the natural sciences, where the efficacy of a technique or technology is the desired outcome. But if one's task is to understand the human activities that go into the production of an institution or organized group capable of producing such outcomes, then an understanding of such a process – in which certain difficulties are faced and overcome in the original work creative of the group – is of crucial importance to us.[11] Since Fleck, many others have studied the actual everyday life activities of research scientists (e.g., Latour/Woolgar 1979; Knorr-Cetina 1981; Ochs et al. 1996; Spinosa/Dreyfus/Flores (1997) in even more ethnographic detail and have come to similar conclusions: naïve descriptions of the scientific method, in which a theory is supposed to stand or fall according to the outcome of a single experiment, are utterly inconsistent with actual laboratory practice.

Central to Latour and Woolgar's (1979) work is their noting of the fact that research scientists continually collect figures, measurements, and that the end product of these figures is often "no more than a curve, a diagram, or a table of figures written on a frail piece of paper... [which] becomes a crucial resource in the construction of a 'substance'" (ibid. 50)[12] – in other words, the end product is, they say, an "*inscription device*." But again, as in Fleck's work, Latour and Woolgar (1979) note that what gets reported in journal articles is only the *product* of a whole lot of detailed interactive work, and the interactive work creative of that product is itself is lost: "One important feature of the use of inscription devices in the laboratory is that once the end product, an inscription, is available, all the intermediary steps which made its production possible are forgotten" (ibid. 63). In other words, Latour and Woolgar (1979) reaffirm Fleck's finding: that the previously incomplete thoughts that

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[11] Kuhn (1979), in his preface to Fleck (1979), remarks that, although he knew of the existence and title of Fleck's book, and it gave him confidence "that the problems that concerned me had a fundamentally sociological dimension" (ibid. viii), "I am not sure that I took anything much more concrete from Fleck's work, though I obviously may and undoubtedly should have" (ibid. ix). And he continues: "Rereading the book now, as I have done in the interim, I find many insights that I might fruitfully have worked into my viewpoint" (ibid. ix). However, Kuhn's account of scientific discovery still prevails (especially in organizational studies) over that of Fleck.

[12] As in the Ochs et al. (1996) account, for example.

motived the actual research *process* – that constituted the "frontier thinking" described by Hanson (1958) – cannot be expressed within the now finished concepts that are a *product* of it. Again, however, we must note that Latour and Woolgar's (1979) work focussed on activities within a successful natural scientific research culture, the neuroendocrinology research laboratory at the Salk Institute; they did not focus at all on groups of people who experienced themselves as subjected to a rigorous, scientifically controlled research process.

In a now classic piece of work, in which he made explicit many of the unintended consequences implicit in attempts to implement rigorous research designs in experimental or field settings, Argyris (1974: 167) noted, among many other findings, that such attempts tended "to place subjects in situations that are similar, at worst, to the low-skill and, at best, to high-skill employees in organizations." And one unintended consequence of this is that subjects can feel 'picked on', 'pushed around', 'likely to found wanting', as well as feeling anxious as to whether the research will have an effect 'on their wages', 'on those they work with', or 'on their personal life' – effects due to the imbalance of power and control researchers have over subjects. "It would make sense," says Argyris (1974: 167), "to provide subjects with greater influence, with longer time perspective regarding, and greater internal involvement in, the research project. It is understandable that researchers resist these suggestions. They argue that all research could be ruined if subjects had greater influence. These arguments are almost identical with the reactions of many executives when asked to consider giving greater influence to their employees in administration of the firm."

Fricke's (1983) *participatory research* can be seen as a response to some of the difficulties raised by Argyris with respect to *controlled* and thus *imposed* research designs. By creating a situation in which it was "not a case of the researchers as outside experts training the workers but rather a situation where researchers and workers learned together" (ibid. 80), Fricke (like Argyris) found that the whole atmosphere changed. Workers, even after decades of work under extreme stress and in unskilled jobs, were willing, interested, and competent to participate in bettering their own working conditions according to their interests. But yet again (as Argyris suggests above), Fricke

(1983) found that – because of the centralization of decision making at management level, the Taylorization of work, piece-rate payments systems, and suchlike – "the very organization of work institutionally disabled workers from using the practical insights and workplace-specific knowledge to improve things" (ibid. 74).

Work of this kind demonstrates the real possibilities for a move away from theory-driven research towards a much more practice-situated, action research approach to human affairs. Indeed, there is a crying need to mobilize the *transitory knowledge* people use in coping with the unique struggles they meet in the actual daily execution of tasks arising in their practices. For they are in a position to tell of their experiences in ways that 'point forward' in ways that are far more 'moving' than can conventional research with its retrospective approach which can only provide 'after the fact' rationalizations of what practitioners have already done.

A project that offers the 'pointing forward' kind of understanding discussed above, is Taptiklis's (forthcoming) current *storymaker project*. He is concerned with inviting people to reflect on their working life within organizations and to recall moments, episodes, or experiences that in hindsight seemed striking in some way. His purpose in doing this – a purpose that arose out of previous research on the struggles people faced in coping with "life events" and the need to help people to 'navigate' themselves through such events – was with the following question in mind: is it possible to find just that fragment of another person's life that can help you decide what to do for yourself?

Influenced by this experience, and by the work of a number of others who had also found that people give shape to their lives in terms of crucial "life events," in particular Benner (1984), Taptiklis began the storymaker project, concerned to amass just such a repertoire of action guiding fragments of relevance to workers in particular institutions. The example I will give is drawn from an inquiry conducted during 2004 with a team of experienced social workers in a national, New Zealand social work agency that had undergone a two-year experiment called a 'strengths-based' approach to child protection. Could the experiences gained be captured in a way that others – novices, perhaps – could benefit from? Here's an excerpt from one of the transcripts, in

which an experienced social worker who talks of how she successfully coped with a child in care, whom she 'felt a lot for', who 'went missing' while she herself was away on leave:

> *Narrator*: ... and she just took off and no-one could find her.
> I read it in the paper, and when I got back to work, and we found her,
> and so I went in and said
> what the *hell* do you think you're doing.
> Don't you *ever* do that again.
> People worry, blah, blah, blah blah, blah.
> I was quite staunch. I said, Just don't do that.
> If you've got a problem...but I could see why she did it, because there
> was actually no-one there for her.
> The caregiver was gone, I wasn't there,
> so she just did what she felt she needed to do. And anyway,
> a couple of days later she gave me *that*
> (indicates greeting card)
> and for this girl to actually say *sorry,*
> who would *never* admit to doing anything wrong,
> showed that it's working.

Portrayed in this vignette is the importance of the social worker's care for the girl, and the girl's noticing of it, so that when it wasn't present, the girl felt alone and abandoned. Although some may protest at the seemingly uncontrolled expression of emotion and of her worry by the social worker towards the girl, its spontaneity is a genuine expression to the girl of her mattering to the social worker – a more controlled telling could easily be insincere. The action guiding power of this vignette is, then, I think, clear. The reformulation of the 'lesson' here in the more formal terms of a protocol, policy, or general rule would leave practitioners in the position of 'getting the picture' but rob them of the *felt* action guiding movements such a vignette as this arouses. The rule or protocol would 'lose the phenomena' relevant to people understanding the meaning of the events in this vignette for them in their practice.

Conclusions

Theory-oriented, academic social science researchers, speaking in seminar rooms and conference halls and writing research papers to be read (and criticized) by their own fellow academics, face a quite different task to that of practice-based action researchers, who must speak with and write for (and sometimes with) those *with whom* they are conducting their inquiries. Thus action researchers, like the research physicists discussed above, must speak and write out *from within* the ongoing activities within which they are currently engaged in such a way that, they do not just 'point out' relevant phenomena to others in the group, they must also 'point forward', prospectively, to the possible next events that might occur – in other words, they must provide not just accurate knowledge but also *actionable knowledge*. On the other hand, academic researchers write about earlier, already completed events that happened when they were involved with those whose activities are now the topic of their talk. And they must produce accurate and adequate linguistic representations of the nature of that activity while looking back at it, retrospectively, but now *from outside* their involvement in it, with the task of describing its form or patterning thus to produce a *theoretical order* that they can claim to have 'discovered' in it. Then next, before being able to 'pass on' their 'theoretical findings' to be 'applied' by the subjects of their research, they must submit their findings to the critical scrutiny of their academic colleagues, who examine whether they are logically justified in making their claims (or not) on the basis of the data they have collected – only if it can pass this 'test' is the claimed knowledge deemed good enough for application.

In the social and behavioural sciences, little theoretical knowledge as such passes through this process unscathed. But irrespective of it surviving this 'gladiatorial' process, could such *representational* forms of knowledge ever be 'applied'? In separating an activity from both the people whose activity it was and from its surrounding circumstances, in the interests of producing general, decontextualized, objective knowledge, theory-oriented and theory-producing researchers must separate the activity studied from the practical

part it played in these people's lives and from its point for them. And rightly so, for this was not their concern: They must locate whatever they say or write, primarily, in their own professional academic context (it must be addressed to those who, especially in promotion and tenure committees, will judge them), and it must direct attention towards what their professional colleagues, with *their* methodological requirements, deem it important to attend to. And *their* aim is to produce *explanatory theories*, i.e., representations of states of affairs that enable those in possession of them *to predict and control the events they represent*. In other words, this is knowledge of use to outsiders concerned to manage and administrate the activities of *others* – a quite different aim from that of producing actionable knowledge for use by those others.

It is no wonder that mainstream social scientists, who are oriented towards their gaining and sustaining a reputation in their journals, seminars, and conferences, have, as the editors of this special issue point out, little knowledge of or experience with action research.

Yet, this need not be the case. It is not as if theory-oriented academic researchers were totally bereft of all kinds of important experiences and understandings arising out of their involvements with those they research on and into outside of the university context. Neither is it the case that their knowledge of theoretical concepts is of no use in illuminating the activities of others: the opposite is the case. But the difficulty here of turning this knowledge in a different direction is not an intellectual difficulty, a problem that can one day be solved by the right kind of discovery, but an orientational or relational difficulty, a difficulty of the will (see the discussion above) – a difficulty that can be overcome by devising ways of communicating what they know to others in forms which *do* 'point forward' to next possible events, ways of communicating which *do* arouse anticipations in other of what next to expect within a particular circumstance.

What I have attempted to do above, then, is to explore some aspects of the nature of research sciences, i.e., of sciences that function to bring new objects of inquiry into existence, with the question in mind: with what kind of science should action research be compared? Where clearly, it is with a research rather than a classical science that I think the appropriate comparison should

be made. Action research would then be seen in a very different light for, as is already very clear, as Hanson's (1958) Wittgenstein-inspired inquiries show, such *research* sciences have a very different structure to them than the 'classical' forms that can developed from them, once the basic nature of their object of inquiry has been established.

But how can it be established? What is it that makes a research science a *successful* research science? Clearly, prior to, and during the conduct their experimental manipulations and the making of their observations, a community of scientific researchers must all be able to communicate amongst themselves in nonmisleading, unconfusing ways *about uniquely new possibilities not yet actualized*; and to do this, they need ways of checking out each other's claims then-and-there, in the ongoing context of their employment. Thus, just as in everyday life situations, scientists also must be able to distinguish between that for which they are responsible, and that which merely happens, irrespective of their agency. For, only if they can sense, when acting in accord with their expectations of what the world might be like, whether the results of their actions accord with, or depart from their expectations, can they ever put them to empirical test. People's sense of their own responsibility for their actions is, then, at the very basis of a research science. Scientists lacking any sense of their own participation in events occurring around them would be unable to do experiments. In other words, scientists in a research science (as well as a classical science for that matter) face communication problems not unlike those faced in action research.

But what is achieved in a successful *research* science as in a piece of successful action research, clearly, is not at all the same as the more tangible achievements (and their derived technologies) attributed to a mature classical science. Successful action research and research sciences give birth to new communities whose members are all oriented in the same, or similar ways, towards seeing events in their surroundings (the situation of their inquiries) as interlinking with each other in anticipated ways, i.e., as inter-related parts of the same unity.

As I see it, then, what is common both to the conduct of (the early stages) of scientific research *and* to action research, is a realm of creative human activity to do with establishing *possibly* new human communities. Within this

sphere, people develop, not only new ways of *relating themselves to each other*, but also as a result, new ways of *relating themselves to the 'other-nesses*' in their surroundings as well. It is in the task of inquiring into the nature of these early stages in the development of such communities that, I think, Wittgenstein's (1953) special kind of *practical* philosophy has its application. For, as he himself puts it, his kind of philosophy "simply puts everything before us, and neither explains nor deduces anything. – Since everything lies open to view there is nothing to explain. For what is hidden, for example, is of no interest to us. One might give the name 'philosophy' to what is possible *before* all new discoveries and inventions" (no. 126).

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